

instance L. 39836, a star which Lalande considered a sixth magnitude.

Different views will be taken with regard to the proper contents of a celestial atlas, intended for general use, and it is not therefore desirable to be too critical upon this point, but to take, we will say, two extreme uses to which an atlas of the pretensions of Dien's may be applied, first for following a small planet with the aid of a chart professing to contain stars to a less degree of brightness, and secondly, for identifying the naked-eye stars by the general maps including only these brighter stars, an elementary purpose for which an atlas may be quite as readily adapted as a globe. In the former case Dien's maps are not sufficiently filled in to allow of a planet equalling in brightness stars of Bessel's ninth magnitude being identified without some trouble and disappointment, and in the latter case we meet with a failing which is only too common with star-atlases—the outlines of constellations are so prominently drawn as seriously to interfere with, if not entirely to obliterate the naked-eye stars of the lower magnitudes, in using the "Atlas" in the open air. As a model of what an atlas should be in the latter respect, we must still refer to Argelander's "Uranometria," which, in our opinion, has yet no equal for the more elementary uses of such a work.

Among the best features in the new edition of Dien's "Atlas" are the delineation of the southern heavens, in which Brisbane's stars are laid down, the view of the distribution of double and multiple stars by M. Flammarion, the orbits of some of the principle revolving double-stars, and figures of remarkable nebulae and clusters of stars.

### OUR BOOK SHELF

*Horticulture.* By F. W. Burbidge. With Illustrations. (London: E. Stanford, 1877.)

THIS is one of the series of small handbooks on the British manufacturing industries, edited by Mr. G. Phillips Bevan, of which we have already noticed several volumes. A compact work on practical gardening, to serve as a guide to the amateur gardener and fruit-grower, was much wanted, and this volume to a certain extent supplies the desideratum. After a short chapter on commercial gardening, the author treats of the cultivation of fruit, and of the various descriptions of vegetables and herbs; and then of gardening in its various departments, but more from the economical than from the amateur's point of view. If the owner of a garden wants to turn his bit of land to the most profitable account, he will find Mr. Burbidge an admirable guide; but if he infers from the title of the book that he will obtain from it advice as to the treatment of his pelargoniums, fuchsias, and chrysanthemums, or the management of his hothouses, he will be disappointed. We fancy that information of this kind would commend itself to a larger number of readers than the guide-book information of the exact number of acres in each of our London parks, and the annual cost of maintaining them. The advice as to the culture of fruit and vegetables seems to us very good; but the rather poor woodcuts do not add to the value of the volume.

*Mittheilungen aus dem k. zoologischen Museum zu Dresden.* Herausgegeben mit Unterstützung der General-direction der königlichen Sammlungen für Kunst und Wissenschaft, von Dr. A. B. Meyer, Director des königlichen zoologischen Museums. Zweites Heft mit Tafel. (Dresden, 1877.)

IN a former volume of NATURE (vol. xiii., p. 464) we have

given some account of the origin of this meritorious work, of which the second portion is now before us. Like the former half of the first volume of the contributions the present section is chiefly occupied with memoirs based upon the collections made by Dr. A. B. Meyer during his well-known expedition to New Guinea and the adjacent islands. Herr Th. Kirsch, the entomologist of the Dresden Museum, commences with two articles upon the lepidoptera and beetles collected by Dr. Meyer in New Guinea. Of the former Herr Kirsch enumerates 167 species, of which 133 belong to the diurnal section. Several novelties are described and well figured. The next article is by Dr. Meyer himself, and gives us an account of a large series of Papuan skulls which he collected on the mainland of New Guinea and in the Island of Mysore, in the Bay of Geeldink. The collection, embracing altogether 135 examples, is, we believe, by far the finest of this branch of the human family ever made, and should, we suppose, lead to some definite results upon that somewhat mysterious subject—the differentiation of the various races of mankind by their skulls. A second article by Dr. Meyer relates to the specimens of anthropoid apes in the Dresden Museum. We cannot say that the photographic plates of the stuffed specimens of these creatures are either elegant or likely to be of very great use, but it is satisfactory to have the vexed question of the identity of the celebrated "*Mafoka*" lately living in the Zoological Gardens at Dresden, and long supposed to be a gorilla, finally set at rest, as is done by von Bischoff's article on its anatomy, which follows that of Dr. Meyer. A memoir on the Hexactinellid Sponges collected by Dr. Meyer in the Philippine Seas, in the preparation of which Herr W. Marshall has given his assistance, concludes this interesting volume, of which we may say that it adds materially to the status of the Dresden Museum, and to the scientific fame of its energetic director.

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications. The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

#### The Radiometer and its Lessons

I AM obliged to ask you to allow me to add a few words, by way of further explanation, to my letter printed in NATURE, vol. xvii. p. 80.

In trying to estimate the effect of the communication of heat between a solid body and contiguous gas, I have assumed that certain simplifying suppositions may be legitimately made, for the most part identical with what are very commonly adopted in discussing the pressure exerted by a gas on a solid in contact with it. That is to say, I have assumed, first, that we may resolve the velocities of the molecules of gas into three rectangular components, one perpendicular to the surface of the solid and the other two parallel to it; second, that we may conceive of the whole number of molecules as divided into three equal parts, one-third moving in the direction of each of the resolved components of the velocity respectively; third, that the mutual pressure between the solid and the gas, and any communication of heat from one to the other, may, for the purpose in hand, be attributed to direct impacts of molecules against the solid surface; fourth, that all the molecules endowed with a velocity perpendicular to the solid surface, and contained within a layer adjacent to this surface of a thickness not greater than the mean length of path, will strike the surface, while none of those which are outside this layer will ever reach it; fifth, that the particles which have struck the solid surface will return from it with an average velocity corresponding to the temperature of the surface, and will retain this velocity until they arrive at the farther side of the layer before-mentioned. It was on the supposition that these are legitimate assumptions that

I spoke of heat passing across a stratum of gas from one solid surface to another "as though there were, in contact with each solid surface, a layer of gas whose temperature is throughout the same as [it would perhaps have been better to have said "determined by"] that of the contiguous solid."

I am fully aware of the ease with which one may be led into serious mistakes by trusting too implicitly to such simplifying assumptions, and also that some of the particular suppositions made above would be inadmissible in a discussion of the general problem of the conduction of heat in gases; but I do not see any fallacy in employing them for the special purpose which I had in view in my last letter, namely, to show why I think that the flow of heat across a thin stratum of gas must be facilitated by diminishing the pressure of the gas. Prof. Osborne Reynolds's argument that "if there were a layer of uniform temperature, no heat would be transmitted," does not appear to me to be applicable to the case in question. It seems conceivable, as an extreme case, that, in a very thin layer of gas, between parallel solid surfaces maintained at different temperatures, the molecular movements might take place exclusively in the direction of the perpendicular to the bounding surfaces. In such a case the particles would move from side to side of the layer of gas with a uniform velocity, though the velocity one way would be greater than the velocity the other way, and heat would be transmitted across a layer of gas having the same temperature throughout. Such a condition, whether practically realisable or not, would, if I understand him aright, be the limiting case in one direction of what Mr. Stoney has called for shortness a "Crookes's layer": the limiting case in the other direction being the ordinary condition of a gas, where the average velocity of the molecules is independent of direction. I venture to think that, in pointing out the results which must follow from the existence of a predominating direction of molecular motion, Mr. Stoney has made a very important contribution to the kinetic theory of gases; and I do not see that his conclusions are in any way invalidated by its being shown that they are not in harmony with "the generally-accepted laws of gases," inasmuch as these laws are deduced from suppositions which expressly exclude the conditions he has investigated.

G. CAREY FOSTER

December 17

ALLOW me to say a few words on what I believe to be the correct theory of the radiometer. This theory was given to me by Prof. Osborne Reynolds during spring of 1875, and I have found it capable of explaining every experiment on the subject with which I am acquainted.

The conservation of momentum is one of the laws of nature which even molecules do not break, and that law puts some restraints on the wonderful things which the shocks of molecules can accomplish. Imagine a vessel full of gas at a certain temperature. The centre of gravity of the gas and that of the vessel are supposed to be at relative rest. Suppose now that I increase the velocity of a certain number of molecules in a given direction, the centre of gravity of the gas will move relatively to the centre of gravity of the vessel, and no number of encounters between the molecules can alter that motion until the momentum has been taken up by the vessel. If in any gas we have a passage of heat in a certain direction, we shall have a propagation of momentum owing to the fact that the molecules move more quickly in one direction than in the opposite one, and no number of encounters can alter that propagation. Where the momentum enters the gas and where it leaves it we observe certain forces. This is Prof. Reynolds's theory of the radiometer. It has been objected that an increased pressure on the cool side of the vanes of a radiometer will counterbalance the force acting on the blackened sides, when the dimensions of the vessel are large compared with the mean path of a molecule, but I do not think that such is the case. The following special case may make this point a little clearer. If the forces on the vanes are counterbalanced, the forces on the vessel must be counterbalanced as well. In the case of an ordinary radiometer the forces reduce to a couple, and I do not see how any crowding of molecules in one part of the vessel more than in another can produce a couple on the vessel. The whole problem is one of conduction of heat. All the experiments made by Mr. Crookes on cups, inclined vanes, &c., admit of the same easy explanation as the fact that when a long and a short wire are connected with the poles of a battery, the current in the shorter wire will be the strongest. In a radiometer with inclined vanes, for instance, the

temperature is the same on both sides, but the gradient of temperature is much larger on one side, and hence more heat will escape on that side. The dimensions of the vessel also have to be taken into account in the same way as the length of a wire has to be taken into account when the strength of an electric current flowing through it has to be calculated. It is difficult to say exactly what takes place within very small distances from the hot surface, but it seems clear that any phenomenon, such as Prof. Carey Foster supposes to exist, must affect the passage of heat in the same way as the force on the vanes. As the careful researches of Messrs. Kundt and Warburg have shown that under great exhaustion the conduction of heat decreases and does not increase, I do not see how an increase in the force can take place.

The scientific world will judge how far Prof. Stoney has succeeded in establishing any new laws on the conduction of heat through gases. In justice, however, to Messrs. Provostaye and Dessains, whose experiments he calls to his aid, I wish to point out that their numerous experiments, with two exceptions, are in entire accordance with existing theories. At the time these experiments were made, no distinction was drawn between convection and true conduction. In order to deduce, therefore, the loss of heat due to true conduction, Prof. Stoney is obliged to subtract the effect due to convection currents. He draws, therefore, a curve representing the loss of heat due to this cause. All his conclusions must stand or fall with this curve, and I am afraid they must fall.

After Professors Clausius and Maxwell had deduced theoretically the coefficient of conductivity for gases, a series of celebrated experiments were made by Stefan, by Nairn, by Plank, by Winkelmann, and last, but not least, by Kundt and Warburg. The influence of convection currents has been fully discussed in these papers and eliminated, and the conclusions arrived at by all these experimenters are fully in accordance with each other and with theory. It appears, as was expected, that when the effects of convection currents are eliminated, the coefficient of conductivity is independent of pressure until the dimensions of the vessel are comparable with the mean free path of a molecule, and that then the conductivity rapidly diminishes. It also appears that at the pressures at which Messrs. Provostaye and Dessains found that the loss of heat was independent of pressure, convection currents must have ceased to be appreciable, and therefore the great mass of their experiments is fully in agreement with later researches.

The only exception is found in the case of carbonic acid and nitrous oxide. These abnormal results were not confirmed by Messrs. Kundt and Warburg in the case of carbonic acid, the only one of the two gases which they examined. Whoever reads their account of the difficulty they had in excluding the last traces of moisture, and considers the increased conductivity which such an admixture would produce as the pressure diminishes, will have no difficulty in explaining the anomaly. At any rate I do not think Prof. Stoney will be inclined to base important conclusions on unconfirmed experiments on two gases in which we should expect the effect, owing to their density, to be particularly small. The discovery of Master Gerald Stoney, who found that a red hot wire was cooled when a tin can containing water was brought sufficiently close might, I think, have been foretold by the recognised theory. Prof. Stoney, no doubt, will find on reading over the literature on the subject, that what he calls penetration of heat, has hitherto been known under the name of conduction of heat, that it takes place at all pressures, and begins to disappear at the exact point at which he makes it appear.

The timely calculation of Mr. S. T. Preston in the August number of the *Phil. Mag.*, shows that any theory of the radiometer which makes the action depend on the comparatively large ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong.

ARTHUR SCHUSTER

#### The Proposed Channel Islands' Zoological Station, Aquarium, and Piscicultural Institute

I AM very anxious that this project should succeed, mainly because of the facilities it will afford to inland aquaria, in procuring living animals cheaper, better, more variedly, and more systematically, than at present. This, I believe, will form the most profitable part of the undertaking.

\* Referred to in NATURE, vol. xvii. p. 102.